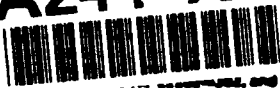


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**INTERIM REPORT**  
**Grant AFOSR 88-0350**

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Princeton, N.J. 08544

During the period of this grant the theory of superconductivity in high  $T_c$  cuprates matured into a reasonably consistent, complete theory which has the capability, often realized, of confronting all of the puzzling experimental properties of the materials. During the period of the grant occurred the Cargèse NATO Summer School (June 1990) attended by several of us who were being funded by the grant, and at that school I summarized progress up to that time. B. Doucot who had been one of our group was the local organizer.

Perhaps the best summary of the situation at that time was given in my "Chapter II"\* setting out what I called the "Central Dogmas" of the theory, which is enclosed. At that meeting was formulated the justification of the Luttinger liquid hypothesis via a finite Fermi surface phase shift which led to several papers, especially the PRL and 'response' on the subject showing how the Fermi liquid theory breaks down.

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\* For a book based on the Cargèse meeting

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## CHAPTER II

### The "Central Dogmas"

At a certain point in the process of unravelling the "Secret of Life"—for which read the mechanisms of reproduction and transcription of biological information—F.C. Crick propounded what he called "The Central Dogma" which constrained the overall structure of any description of the actual mechanism. The Central Dogma was determined by logical deduction from the overall experimental facts of biology. The very important conceptual function which was played by the "Central Dogma" was to limit serious discussion of mechanisms and theories to those which were consistent with logic and with the overall burden of experimental fact, while allowing a great deal of freedom in working out specific mechanisms, and leaving the overall structure of the theory immune to changes in specific processes. For instance, it was "dogma" that a genetic code existed, but the theory was independent of details of that code.

The main function of this book is to convince the reader that such a system of "dogmas" is useful for the field of high  $T_c$  superconductivity, a field which has the same kind of complexity and confusion as microbiology had at that time. As in molecular biology, there is enough irrelevant complexity that an unwitting theorist may never reach the neighborhood of the actual problem, even though he is working along a line which is widely represented in the literature. Understanding high  $T_c$  involves not one, but a multiplicity of steps, and it is vital to provide a map through the maze of alternative paths, almost all of which can be eliminated by simple logic using simple and well-founded experimental or theoretical reasoning, of a sort which should be immediately persuasive.

A second reason for propounding these "dogmas" is to correct the general misapprehension that there is no viable theory of high  $T_c$  superconductivity, a misapprehension so widespread that even the President's science advisor has complained publicly that research is hampered by the lack of a theory. The problem is not the lack of a theory but its com-

plexity and the fact that it consists of a number of steps of widely different completeness, involving different types of arguments, published in a bewildering variety of places and versions, and embedded in a literature of great complexity which is beset with controversy. A real problem is that many physicists nowadays are unfamiliar with the process of rigorous deduction from theoretical concepts combined with a broad range of experimental facts, a process which is the primary source of the rather complete and consistent picture we now have. (As it was the primary source of the Fermi liquid theory of real metals which it replaces.) The problem is not one theory but too many theories, many of which contain germs of truth because of tying in to a single valid experimental fact or theoretical concept, but which do not take into account the key requirement of overall consistency with the complete picture. Aside from the theoretical papers which have some germs of truth in them, there is a much larger group which are completely inconsistent with the basic realities of the subject. Most of the papers written in this field are not just easily falsified, in the Popperian sense, but actually falsified before they start: "previously falsified." I think the problem is one of a new kind of scientific sociology: physics has become so specialized and fragmented that an attempt at overall consistency with the observed facts and fundamental restrictions of theory is not seen as a necessary precondition for publishing a theory.

We feel the only possible presentation of the complete picture is to give the overall view, step by step, postponing as far as possible the details, such as precise justifications, alternative techniques, and detailed critiques. The resulting dogmas take us step by step through the process of solving a typical, if somewhat difficult, problem in quantum condensed matter physics. Typically, one has to go through several stages of "renormalization", which is a fancy word for abstracting the relevant parts of the problem and eliminating the irrelevant high-energy degrees of freedom. This is why it is a canard that such a problem can be solved by enough computing power: it is hopeless until one has gone through this process. The most important terms by far are those which open up gaps in the spectrum, because states above any such gap can always be eliminated exactly and

replaced by effective interactions among the remaining low-energy, low-frequency degrees of freedom: a process discovered by Van Vleck many decades ago. The whole problem of high  $T_c$  is a lesson—almost a poem—in restricting Hilbert space.

To summarize what we shall do, the first three of the six dogmas have to do with such eliminations. The first two restrict our attention to a single band of the one-electron spectrum, the antibonding, " $d_{x^2-y^2}$ " symmetry band on the  $\text{CuO}_2$  planes. Other bands are separated by large energy gaps from the relevant degrees of freedom near the Fermi level, and can be eliminated: the only relevant Hilbert space is this band. The third dogma tells us that only one of the interactions of the electrons in this band is so large as to open up yet another gap and further restrict Hilbert space. The next two dogmas are descriptions of the state of the "normal" non-superconducting metal which we encounter when we enforce these restrictions, both of which are almost equally supported by experimental and theoretical arguments. The first states that the resulting metal is in an unconventional state which we call a "Luttinger liquid", possessing a Fermi surface but no conventional electron quasiparticles; and the second that this state is strictly two-dimensional in this case. Only the last dogma, then, has to do with the superconducting state: it tells us which of the residual interactions which are left can be strong enough to give us the unconventional high transition temperatures which are observed. Again much of the argument is from experiment.

In fact, we will show that there are at least two levels of certainty. The reader may, at this point, leave out one or more of the second level of dogmas, which we shall call "dictats", if he can find an acceptable replacement mechanism; but he ignores the more basic "dogmas" at his peril. If he chooses to do so, he will get no value from this book. Let us then set out this list of "dogmas" and "dictats" with some discussion of the basis behind each and of alternatives which have been proposed.

**Dogma I:** All the relevant carriers of both spin and electricity reside in the  $\text{CuO}_2$  planes and derive from the hybridized  $\text{O}_{2p} - d_{x^2-y^2}$  orbital which dominates the binding in these compounds.

The main alternative was the "chain" school, but now the one compound, YBCO, which has chain coppers, is in a tiny minority among some dozen compounds, none of the rest of which have chains and all of which behave with remarkable similarity. The infrared data on single untwinned crystals of Schlesinger and Collins show with remarkable conclusiveness that chain conductivity is qualitatively different and relatively little affected by superconductivity. A more persistent and subtle fallacy, which this dogma excludes, is the literal acceptance of band theory results which often give bits of Fermi surface attached to other parts of the structure: the notorious "bismuth pockets" in BISCO, for instance, predicted by Freeman et al and discussed in ARPES papers, or the Fermi surface in the chains. The *c*-axis resistivity in BISCO,  $10^5$  times that in the planes, means unequivocally that no essentially 3-dimensional pockets of carriers can exist in that case. The band calculations, based as they are on an idealized, stoichiometric structure, are also incompatible with the graphite-like cleavage between the *Bi* layers, which shows that no bands near the Fermi surface are occupied at *Bi*. No band calculations are to be trusted at this kind of level of accuracy, and the real band structure must be deduced from experiment.

The most commonsensical approach to Dogma I is to recognize the many anomalous properties of the cuprate layer materials, which are unique to those materials; and the rather unique chemistry of the cuprate layers, involving an unusual valence of *Cu* and very strong Jahn-Teller distortion and semicovalent bonds. The "Anderson mystery story principle", which is the original source of most of the dogma, then operates: this principle is that we must associate all truly unusual events with each other and with the basic problem.

Dictat#1 : Excitations outside of the planes, and probably even non-bonding or bonding bands in the planes, play very little role and can be ignored except for minor renormalizations. The source of this is the remarkable similarity in behavior of materials with widely different chemistry outside the planes: for instance, the *ab* plane normal state resistivity per plane doesn't vary by more than a factor 2 among 5 or 6 compounds. Normal state



infrared and tunneling data are also very similar. Only  $T_c$  varies widely, a fact which we will discuss later.

Dogma I in summary: look at the planes only (a great and welcome simplification.)

Dogma II : Magnetism and high  $T_c$  superconductivity are closely related, in a very specific sense: i.e., the electrons which exhibit magnetism are the same as the charge carriers.

The initial source of this was the "generalized phase diagrams" of state vs. doping, which can be traced out in several compounds.  $\delta = 0$  (pure  $Cu^{++}$  oxidation state) is an antiferromagnetic insulator, with relatively high  $T_{Nee}$  if not frustrated: it is a straightforward Mott-Hubbard insulator with at least a 2 volt charge-transfer gap. (It is regrettable that the advent of mindless band calculation techniques, among other influences, has led to the misapprehension that Mott insulators are (a) controversial and (b) rare.) The present view is that a relatively sharp transition occurs at  $\delta \sim .05 - .1$  to a metallic state which almost always is superconducting, with initially finite  $T_c$ .  $T_c$  seemed originally to rise continuously with  $\delta$  from the insulator but this has not been demonstrated clearly. This metallic state is always peculiar as we shall later discuss; when it turns into a normal metal, with excessive doping,  $T_c$  goes down. (This is probably caused by occupancy of additional bands.)

Since in (almost) all other substances low carrier number, metal-insulator transitions, and antiferromagnetism decrease  $T_c$ , the mystery story principle requires the association.

A theoretical point: the effect of doping on Mott insulators has been a controversial and unsolved problem for decades; again the overwhelming temptation is to associate difficulties.

More straightforward, and equally logically compelling, are optical, photoelectron spectroscopy, and NMR data. From optics and PES it is clear that the carriers appear in the Mott-Hubbard gap in proportion to the doping. NMR data show that the hyperfine couplings of the metallic carriers are identical with those of the spins responsible for magnetism.

Theory—so long as optics, PES, and other probes confirm the presence of the new

carriers in the same orbitals as the magnetism—is equally compelling. The strength of the semicovalent bond due to  $O_{p\sigma} - d_{x^2-y^2}(Cu)$  hybridization is responsible for the great integrity of the square planar configuration of  $CuO_2$  planes. This suggests that, next to the  $Cu^{++}$  “ $U$ ” repulsion, the second largest parameter is the  $2p\sigma - d\sigma$  hybridization  $t_{dp}$ , which must be of order 2 – 3 eV, leading to the observed  $\sim 6$  eV splitting of bonding and antibonding bands. Between these two levels are a spaghetti of weakly-bonding  $Cu - O$  hybrids which form a very large hump  $\gtrsim 1 - 2$  eV below the Fermi level. The antibonding band has only one state per  $Cu$  ion, and therefore the appropriate Hilbert space for leaving the magnetic state intact and introducing a new set of carriers does not exist.

A considerable body of reliable electronic structure calculations by Schluter and others confirms this picture and gives us reasonably reliable values for the Hubbard “ $U$ ” and “ $t$ ” parameters.

The most persistent fallacy evading Dogma II is the “extended Hubbard Model” and various variants thereof, which are at least formally correct in that they can be reduced to the right model unless they have the wrong parameter values, but are an unnecessary detour of no physical value. More naive theories simply don’t question where, electrically, the magnetism comes from, and use coupled magnetism-carrier physics. Various probes show that the magnetic and charge form-factors of the carriers differ somewhat: they are  $\gtrsim 60\%$   $d$ , at least, magnetically, and  $\gtrsim 60\%$   $p$  electrically. This can be understood as different polarization of the background bands by exchange and Coulomb interactions, by a single band of renormalized carriers. Thus the conclusion is:

II: We must solve the old problem of doping a single Mott-Hubbard band before we can begin the problem of high  $T_c$ . After renormalizing away high-energy excitations, the physical particles live in a single band. The problem is the very old problem of reconciling their magnetic structure and their charge transport.

**Dogma III:** The dominant interactions are repulsive and their energy scales are all large. Clearly the existence of a Mott half-filled band insulator implies large repulsive interactions whose scale may be bounded below by the Mott-Hubbard gap for charge transfer. A

second scale is set by the exchange parameter which, by various accurate experimental measurements, especially spin wave velocities and Raman spectra, is at least 1200°K, leading to spin wave bands .2 – .3 ev wide and a spin wave velocity comparable ( $\sim 1/4$  at least) to Fermi velocities. In Hubbard model terms, we are in the case of large but not infinite  $U$ .

Many clear indications place the intrinsic electron-phonon coupling at normal to large. Most striking are the shifts of optical phonons associated with the gap, shown in Fano resonances with the anomalous electronic background in the Raman effect. There is no reason to doubt various direct electronic calculations of these couplings. What is striking is that they have so little effect. We believe the resistivity which would have been caused by phonons and by static lattice distortions and defects in the (non-stoichiometric) normal metal, is much larger than the observed  $\rho_{ab}$ , which—again—does not vary from substance to substance as much as a phonon resistivity would. Equally, “phonon bumps” do not show up strongly in tunneling, infrared, and ARPES spectra. This situation reveals one of the crucial anomalies of the high  $T_c$  materials. We can and will describe the physics by saying that the strong repulsions dominate and restrict the response of the charge and spin density fluctuations to phonon and static potentials, but the formal theory of this experimentally well-attested fact is rather heuristic, a “gedanken theorie” to quote Bob Schrieffer.

A little more may be said. We are accustomed to the fact that collective, bose-like modes (phonons, plasmons, spin waves) are much less easily scattered at low frequencies and long wavelengths than particle wave functions. In the “Luttinger liquid” type of theory, for charge transport the particle modes have been replaced by the collective motions, by the simple construction dating back to Tomonaga: these experimental facts support this kind of electronic theory. For instance, the heat conductivity suggests that the phonons are strongly scattered, if not vice versa. How can this happen? Experiment, rather than theory, then tells us:

III: Restrict your attention to a single band, repulsive (not too big)  $U$  Hubbard Model.

Dogma IV. The "normal" metal above  $T_c$  is the solution of the planar one-band problem resulting from Dogma III, and is not a Fermi liquid, in the sense that  $Z = 0$ . ( $Z$  being the quasiparticle wave-function renormalization constant.) But it retains a Fermi surface satisfying Luttinger's theorem at least in the highest  $T_c$  materials. We call this a Luttinger Liquid.

This has several sound experimental and theoretical bases. The most vital experimental evidence lies in the giant anisotropy of resistivity  $\rho_c$ . The resistivity perpendicular to the planes extrapolates to  $\infty$  at  $T=0$  with an exponent  $T^{-1\sim 2}$  and is in all cases well above the Mott limit  $\frac{h}{e^2 k_F}$ . This means that there is no coherent electronic transport in the  $c$ -direction: all motions are inelastic. Fermi liquids cannot localize in one direction and extend in a second, because localization is a question of coherence: are the electrons coherent in extended or in localized states? Thus a fortiori the normal state is a two-dimensional metal with only inelastic processes connecting the layers; this means, incidentally, that not even normal Giaever tunneling is taking place, which can be shown to be a strong argument against single electron quasiparticles being the elementary excitations.

In these experimental considerations and also in later ones a theorem due to Schrieffer (as far as I know) is very important: single-particle tunneling and transport are not renormalized by the wave-function renormalization constant " $Z$ ", in any conventional—or even unconventional, as in the case of weak localization—Fermi liquid theory. This was discovered in relation to superconducting tunneling and in the verification of phonon interactions in BCS theory it played a vital role, but it was equally important in the hands of Mott, Thouless, and other early workers in localization theory, who recognized that the conductance  $e^2/h$  is a universal boundary between metallic and insulating states, independently of dynamic effects, because conductivity is not renormalized. The modern ideas on conductivity using the  $S$ -matrix, pioneered by Landauer, give us considerable understanding of the universality of this theorem.

Several authenticated cases of "insulating" transverse conductivity exist in the literature in other systems (e.g.,  $TaS_2$ ) and we would suggest that these be re-examined,

since this behavior will not normally occur in a Fermi liquid. If the matrix elements are genuinely tiny, it is possible that inelastically assisted conductivity could dominate, but in all high  $T_c$  materials but BISCO the observed 3d superconductivity rules this option out (see later).

A second very strong argument is the small value of  $\rho_{ab}$  and the relative sharpness of the features in the ARPES spectrum. Reasonable estimates of impurity scattering by the large non-stoichiometry in (214) or BISCO leads to a mean free path  $l \sim$  a few  $\times a_0 \sim 20\text{\AA}$  while  $\rho_{ab}$  corresponds to roughly  $l = 50 - 100\text{\AA}$  at  $T=100^\circ\text{K}$ . Any reasonable estimate of phonon scattering also lead to a bigger  $\rho_{ab}$  than observed. The ARPES peak widths are in good agreement with  $\rho_{ab}$  while the feature sharpness is even smaller. Hence, giant concentrations of charged impurities have no effect on  $\rho_{ab}$ ; tiny percentages of substitutional uncharged impurities in the planes, on the other hand, which carry free bound spins according to several measurements, lead to reasonable residual resistances and to  $T_c$  lowering.

We can only conclude that current is carried by some collective or soliton excitation whose motion is controlled by uncharged entities, i.e.,  $Z = 0$ ,  $F_{OS} = \infty$ , Fermions as we shall shortly discuss. A good model for such a state and such excitations is given by appropriately reinterpreting the exact Lieb-Wu solutions of the one-dimensional Hubbard model, but why this should work so well for the two-dimensional system is still under discussion.

$Z = 0$  is confirmed by the several measurements, all of which agree, of inelastic scattering  $\tau$ 's: infrared, Raman background, ARPES. All tell us that  $\frac{1}{\tau} \propto \omega$ , and if we treat the carriers as quasiparticles with a self-energy  $\Sigma$ ,  $\Sigma_{im} \propto \omega$  and  $\Sigma_{re} \propto \omega \ln \omega$  by Kramers-Kronig transform. Thus  $\frac{\partial \Sigma}{\partial \omega} \Big|_{\omega=0} \rightarrow \infty$  and  $Z = (1 - \frac{\partial \Sigma}{\partial \omega})^{-1} \rightarrow 0$ . One's first response to  $Z = 0$  is to abandon entirely the Fermi liquid quasiparticle theory on which this derivation is based. However, we shall see that both the one-dimensional model and the experimental facts show that the Fermi surface and the Fermion excitations may remain even when electron-like quasiparticles are absent.

There is a strong theoretical argument and motivation for this peculiar non-Fermi liquid normal state. The one-band Hubbard model has the property of having the "upper Hubbard band", a separated band of states thought of as comprising the motions of electrons on doubly occupied sites. This can be given a precise meaning in at least two ways: as a band of separated "anti-bound" particle-particle scattering states, or by the Rice-Kohn-Anderson canonical transformation procedure to the "t-J" model in which the kinetic energy term is exactly projected onto an equivalent singly-occupied subspace. The key operative word is "projective": the Hilbert space of the new low-energy problem is smaller than that of the corresponding Fermi liquid states, because projection operators have zero eigenvalues. This change of Hilbert space means that the states of the  $N + 1$  body problem live in a new Hilbert space and are necessarily orthogonal to those of the  $N$  body problem, hence  $Z$ —which is a ground state to ground state overlap integral—is zero.

Intrigued by this problem of doping of the Mott-Hubbard insulator, theorists have rather ingeniously found at least three viable alternative approaches, and we take it as at most a "Dictat" that one should follow only one of these—but as dogma what the final result must be like.

Two possibly valid but somewhat indirect approaches are twisted antiferromagnetic order parameters and "flux" or "anyon" phases. A few carriers doped into the antiferromagnetic insulator can be shown rigorously to generate a co-moving distortion ("twist") of the antiferromagnetic order parameter, and the resulting soliton may be a model for our  $Z = 0$  object—it is an excellent one in 1-dimension. A second concept is to model the Mott insulator with an "RVB" liquid of short-range singlet pairs, and to study solitons in this; again, a limiting process starting this way is a way of thinking about 1d. The resulting particles carry gauge fields in order to implement the projective transformations. But experiment seems, in most cases, firmly to reject the short-range picture. Yet another method is to follow the original suggestion of Anderson and Zou modeled on the short-range RVB ideas and to implement the projection with a "slave boson" (or

“slave Fermion”) theory with spinons, holons, and gauge fields, calculating directly using the full gauge theory. Lee and Nagaosa, and Yoffe and Wiegmann, calculate in this way without assuming short-range RVB, and in particular Lee assumes a Fermi surface for his spinons. Many properties can be well calculated. A fourth concept, closest to the original picture of Anderson and BZA, is a  $Z = 0$  liquid of spin 1/2 Fermi-like particles, and charged  $S=0$  excitations or holons. Perhaps the most straightforward way to describe such a fluid is as a limiting case of the Fermi liquid, such a system as one might find if—as in 3 dimensions—there is a  $U_{crit}$  dividing Fermi liquid from Mott insulating states. As one approaches  $U_{crit}$   $Z \rightarrow 0$  implying  $\frac{\partial \sum}{\partial \omega} \rightarrow \infty$ . However, we recognize that there remains a finite spin velocity in this limit, hence the Fermion mass does not go to zero. This is only possible if  $\frac{\partial \sum}{\partial \omega} / \frac{\partial \sum}{\partial k} = \frac{1}{v}$  remains finite, hence the compressibility which is proportional to  $(\frac{\partial \sum}{\partial k})^{-1} \rightarrow 0$  and the Landau parameter  $F_0^S \rightarrow \infty$ . Thus charge cannot be carried by the Fermions, any Fermion being perfectly surrounded by compensating charge in the medium. It must reside in collective sliding motions of the Fermion liquid, or bose like charged excitations which we think of as a second, charged soliton or “holon”. All of this picture is precisely modeled on the one-dimensional case which is exactly soluble.

Several experimental facts drive us to this type of theory. The Pauli-like spin susceptibility (except in the anomalous case of  $Y(Ba)_2Cu_3O_{6.65}$  which has many unique properties) is that of an equivalent spinon liquid. Most strikingly, the existence of a Fermi surface in the ARPES measurements on the normal state can only be understood this way (the interpretation of ARPES energy distribution data certainly does not contradict, and possibly strongly supports, the “Luttinger liquid” picture; but this controversial subject is best bypassed as far as possible).

One of the methodological strengths of condensed matter physics is the overdetermination by data. It is often the strongest evidence for a theory that none of the wide variety of possible probes contradicts it; let us assure you this is the case here.

The main alternative theory, which is excluded by the evidence for Dogma IV, is any of the many versions of conventional or unconventional Fermi liquid theory. The data require

that the electron excitation be composite, not elementary, and, in fact, that it decay at a rate given by the available phase space. This type of behavior of  $\rho_c$  is seen elsewhere only in conjunction with some kind of "unusual" condensed state such as SDW's or CDW's in dichalcogenides or organics, where the charge carriers are coupled to an order parameter. And the existence of the upper Hubbard band, which drives the  $Z=0$  process, is clear in optical data.

The "marginal Fermi liquid" theory is an alternative path which uses much of the above experimental argument but attempts to distance itself from the Hubbard model. It is true that attractive models can also have varieties of non-Fermi liquid behavior in 1 and 2d systems. The primary distinction is the rejection of the concept of spinons and of the spinon Fermi surface without any consistent alternative being proposed. The evidence which is relevant is that for Dictat I, Dogma II, and the upper Hubbard band. Our frank assessment is that the most valid parts of MFLT are those which are borrowed from the main line of reasoning. There is one interesting question: assuming, as we must, that  $Z = 0$  implies charge-spin separation and the spinon Fermi surface, is it still necessarily true that there is a true holon excitation, or could the charge be carried by a collective resonance near the  $2k_F$  edge of the spinon pair spectrum? Calculations aimed at proving the existence of a bound  $2k_F$  charged collective mode have not succeeded; yet it seems strange to make up a charged resonance from neutral Fermions! This is a serious question for study. We summarize Dogma IV by the statement: the normal metal is a two-dimensional Luttinger liquid: i.e., Fermion-like spin excitations—spinons—establish a Luttinger Theorem Fermi surface. Charge is carried by an alternative excitation. Charged excitations can not be thought of as having fixed statistics and none of the three: electron, spinon, holon, is a simple bound state of the other two ( in contradiction to short-range, KRS RVB and to PWA's Varenna notes!)

**Dogma V** : The above state is strictly two-dimensional and coherent transport in the third dimension is blocked.

**Dictat V** : This two-dimensional state is not superconducting and has no major interac-



tions tending to make it so, at least near the usual  $T_c$ . For the Dogma, the often cited c-axis resistivity would be argument enough, but there is a second important experimental one. The ARPES data on BISCO 2212 resolve a very sharp cusp feature near the Fermi surface, and an even-sharper quasiparticle peak below  $T_c$  in a variety of directions presumably moving through the Fermi surface. Because of the pair of close  $\text{CuO}_2$  planes in BISCO 2212, the Fermi surface in the planes should split in two by an amount equal to the effective interplane hopping integral  $t_\perp$ , and this effect is indeed to be seen in the calculated band structures. The relevant splitting is a couple of tenths of an eV. At some symmetry points, the splitting is small because the hopping effectively takes place via the  $\text{Sr}^{++}$  ions between the  $\text{CuO}_2$  planes, and hence the effective matrix element can be frustrated. Nonetheless, enough directions have been probed to indicate strongly that this odd-even splitting of the  $\text{CuO}_2$  planar states doesn't exist.

Theoretically—here we have only the one-dimensional model to use as an analogy. Several workers in the heyday of one-dimensional physics pointed out that interchain hopping, if weak, renormalizes to irrelevant at  $\omega, k \rightarrow 0$ . (More recently, D.-H. Lee and ourselves have come to the same conclusion.) Thus there exists a critical value of the hopping,  $t_\perp$ , between  $\text{CuO}_2$  layers, below which the physics of the Luttinger Liquid can remain strictly lower-dimensional.

This is, however, strictly a one-dimensional result depending on the small exponent  $\alpha$  in the Green's function, while we feel that the physical effect is larger and more obvious. If the Luttinger liquid dogma is correct, the electron is not a stable excitation in the plane any more than a quark is in free space: its charge and spin move off immediately at different velocities. Thus it has no  $\omega = 0$  amplitude: it cannot find a stable eigenstate with the same quantum numbers of charge, spin and momentum, which is the prerequisite for coherent motion. Thus theoretically dogma IV seems to lead automatically to dogma V.

The impact of Dogma V, then, is that the two-dimensional state has separation of charge and spin into excitations which are meaningful only within their two-dimensional

substrate; to hop coherently as an electron to another plane is not possible, since the electron is a composite object, not an elementary excitation.

The Dictat that the two-dimensional system is not superconducting is based on the scale and the variability of  $T_c$ .  $T_c$  varies from 0 to 6 to 125° depending on the overall 3-dimensional structure: except for  $\rho_c$ , the only highly variable experimental parameter. This is a scale which is much smaller than the scale at which the Luttinger Liquid properties set in, which is not less than  $\sim 1000^\circ$  K. The properties of the planes have become reasonably uniform, and dominated by strong interaction effects, from this temperature downward. The question is—what could possibly make  $T_c$  be of an entirely different, and widely varying, scale? It seems almost required that  $T_c$  itself depends radically either on interactions between planes or with the substrate, and is not a purely 2 dimensional effect. When superconductivity in fact ensues, the properties seem indeed to become more isotropic, and the penetration depth  $\lambda_c^2$  which is basically a measure of the c-axis plasma frequency changes to a value which is much too high vis a vis the c-axis resistivity.

There are simply no indications of a unique, two-dimensional type of superconductivity. We go into this point more fully, since it is true that 2d fluctuation effects of a fairly conventional type exist above  $T_c$  in YBCO and in fact in all the "multilayer" systems such as Bi and Tl 2212. We propose that pairs or triplets of layers do become superconducting by dint of their interlayer coupling and behave like a single conventional superconducting layer. This is quite different physically from "anyon superconductivity" within a single layer. The Kosterlitz-Thouless behavior often seen is consistent with this idea.

It is possible that the various proposed 2-dimensional superconducting states are in fact nothing but the  $Z = 0$  Luttinger Liquids of Dogma IV in a new guise. No attempts at a fluctuation or thermodynamic theory of "anyon superconductivity", for instance, have been made, only a ground state is discussed. Thus a real possibility is that  $T_c$  is zero or the state is sensitive to impurity scattering of one type or another. The anomalous  $\rho_{ab}$  conductivity seen in experiments extrapolates to zero and hence, in some sense, the state really is "superconducting"; with  $T_c = 0$ ; but some other  $T_c$  mechanism intervenes,

probably that of Dogma VI.

**Dogma VI** Interlayer hopping together with the "confinement" of Dogma V is either the mechanism of or at least a major contributor to superconducting condensation energy.

There is better than a rough identity between the conductivity per electron in a given direction and the total kinetic energy: each are proportional to a velocity-velocity correlation function. Kohn has shown that in a single tight-binding band the frequency integral of the conductivity is proportional to the mean kinetic energy per electron

$$\int \sigma(\omega) d\omega \propto \langle t_{ij} c_i^\dagger c_j \rangle$$

Along the *c*-axis there is a great defect in conductivity: there is no coherent motion of electrons in the *c*-direction. This means that there is, in the normal state, a missing energy of order  $t_\perp^2/t_\parallel$ , which is regained in the superconducting state (since, experimentally, the superconducting response function in this direction appears to be consistent with band structure, and this measures the restoration of the low-frequency part of  $\int \sigma(\omega) d\omega$ ).

There is, therefore, a contribution to the condensation energy which is not a theoretical but an experimental fact and which comes from the interlayer tunneling energy. It is compelling to identify this as the source of the condensation energy, but, of course, pairing has other consequences and may well gain energy from phonon and other modes as well.

The heuristics of transition temperatures lend strong support to this view. We have published a study of relative  $T_c$ 's of Bi 1,2, and 3-layer materials, as well as *Tl*, on this basis. Pressure coefficients are very favorable: these are large of the right sign in all 1-layer materials, but for two or more strongly-coupled layers one cannot be sure what pressure will do. The one mystery is *Tl* one-layer, which has a very mysterious chemistry, fluctuating from zero to 60° with physical state. We suspect that *Tl* orbitals are close to the Fermi level. The fact that this  $T_c$  is so sensitive to interlayer chemistry is in itself mysterious if  $T_c$  is intralayer. We also point out (stimulated by a remark of S. Trugman) that the fact that the top layer of an YBCO crystal seems not to be superconducting is very strange on any interlayer theory. We suspect that YBCO surface problems are caused by the lack

of a satisfactory cleavage in this crystal separating the pairs of layers at the chain lattice planes.

The mathematics of "confinement"—the blockage of interlayer coherent motion—is not complete but is becoming so.

On mechanism, there are no plausible competing theories. It is hardly necessary to detail this. It may be necessary to point out that the "BCS mechanism" has quite a subtle heuristics which was well understood by Morel and Anderson following Bogoliubov within 3 years of BCS. No other "theory" of high  $T_c$  is beyond the level of the Cooper model calculation, using vaguely described "attractive forces" of some hypothecated kind with no chemical or structural heuristic whatever. What is more, no hint of these mechanisms appears in well-designed experiments.

The mystery story principle requires that a unique consequence—high  $T_c$ —be associated with the only other truly unique properties:  $Z = 0$  Fermi liquids, and exact two-dimensionality. In summary, then, Dogma VI tells us that whatever the normal state physics, we can count on the mechanism of interlayer tunneling deconfinement to account for  $T_c$ .

A parenthetical remark: the anomalous case of  $YBa_2Cu_3O_{6.85}$  (the "60° superconductor") has attracted a lot of attention. It may well be that this spin liquid is a real example of one of the other alternative states, with spirals or fluxes. All such liquids have the  $Z = 0$  syndrome and charge-spin separation, so all are equally subject to the same interlayer mechanism.

These six dogmas lead us through a rather intricate maze of alternatives to a consistent, coherent view of the whole fascinating phenomenon of the superconducting cuprates. The data-overdetermination of condensed matter physics tells us, actually, that if a crucial countervailing experiment existed we would surely not be able to sustain such a heavy structure.

There is, however, a set of experiments which can confirm it beyond the shadow of a doubt: and, in preliminary fashion, partially they have done so. These are of three kinds.

(1) Careful infrared and superconducting studies of the  $c$ -axis electromagnetic responses, especially in 214 or other one-layer materials. If (VI) is right, the conductivity which is missing in the normal state should partially reappear in the superconducting response  $1/\lambda^2$ . Infrared can confirm the absence of an appropriate  $\sigma_c(\omega)$  in the whole relevant range in the normal state.

An experiment which can be quite interesting would be to search for the " $\sigma - \pi$ " interband absorption in 2-layer materials in the  $c$  direction. There is, in every band calculation, a splitting of the Fermi surface, due to  $t_\perp$ , of  $\sim .1\text{ev}$ , essentially between bands even and odd in the  $Y$  or other ion between the close  $\text{CuO}_2$  planes. This should, in Fermi liquid theory, cause a very strong  $c$ -active infrared absorption, which should be easily visible. In fact, it should also have a very strong tail towards low frequencies because of the vanishing of this splitting in symmetry directions. If it is not there our diagnosis of "confinement" is confirmed very strongly. The absence of  $\rho_c$  can be thought of, naively, as some kind of localization effect, but the strong reduction of electronic conductivity in a wider frequency range would be very telling.

As with the other experiments we will discuss, these electromagnetic anomalies will manifest themselves as an apparent failure of a sum rule: the superconducting response  $1/\lambda^2$  will not be equal to the integral up to the gap frequency of the normal conductivity.

$$\left(\frac{1}{\lambda^2}\right)_c \neq \int_0^\Delta \sigma_{\text{norm}}^c(\omega) d\omega$$

This is a striking prediction which may already have been verified.

(2) Careful experiments on tunneling along the  $c$  axis. "H I H" junctions consisting either of single crystals clamped together, relying on a surface inactive layer, or true tunnel junctions, for instance created by MBE, should manifest extremely interesting properties. The same arguments needed to explain  $\sigma_c \simeq 0$  seem to lead to weak tunnel currents as well, in this case. We should see a strong contrast between the normal and superconducting states. In the superconducting state Andreev scattering allows quasiparticles to move freely between planes; spinons pick up charge from the condensate and turn back into electron quasi particles. Thus tunneling, at least in the energy gap region, will be relatively normal.

(Of course, energies above the gap are not affected). Thus we can expect to see a rather clean superconducting tunneling spectrum complete with gap and Josephson supercurrent in junctions which appear more or less insulating in the normal state. This is a tricky but easy experiment. I suspect that such junctions have already been observed and discarded by a number of experimentalists.

It also may be relevant to reexamine really clean  $c$ -oriented normal to superconductor contacts—why are they so bad? Do they also show changes below  $T_c$ ?

(3)  $G_1(k, \omega)$  as measured by ARPES, like the electromagnetic response, will probably not obey the usual sum rules. When the sample is normal, the quasiparticles are composite and  $G_1$  is spread over a broad spectrum. We have shown elsewhere that when the system becomes superconducting the quasiparticle amplitude becomes finite and peaks reappear in the Green's function. Again, the effect is an apparent failure of the sum rule for quasiparticle amplitude: the peaks appear to arise from nowhere, because they are borrowed from a wide range of energies. There is a difference, however, in that in this case the original sum rule is still satisfied if all frequencies are summed over.

In the above we have given a logically consistent overall picture of the physics of high  $T_c$ . At the very last stage we find that the picture is susceptible to several clear experimental checks, involving phenomena inexplicable on other grounds. Some other vital areas of experimental study can also be suggested: clearly high-resolution, careful ARPES will continue to be of great value. A less obvious but important area is doping the planes with spin impurities, which by smearing the spinon Fermi surface will rapidly destroy the unique Luttinger liquid properties, reintroducing residual resistance and metallic transport along the  $c$  axis as well as reducing  $T_c$ . In subsequent chapters we will flesh out these ideas. We will focus on transport, experiment and theory; on spectroscopy, Raman, IR, and PES; and then on theoretical concepts: the 1d Hubbard model, Luttinger liquids in higher dimensions, and finally the interlayer mechanism for superconductivity.

## CHAPTER III

### Normal State Transport Properties in the High $T_c$ Superconductors: Evidence for Non-Fermi Liquid States, and Attempts at Calculation

Almost the first striking measurements of high  $T_c$  superconductivity revealed a structure of resistivity vs. temperature which showed unusual features in the normal state in addition to the high transition temperature. Well-made ceramics characteristically showed a "metallic" normal state with  $\rho$  linear in  $T$  with a very small  $T = 0$  intercept; while poorly compacted samples, showing rather poorly metallic or almost insulating properties in the normal state, still showed superconductivity easily. With the advent of good single crystals and films, we now know that both kinds of behavior are characteristic. The resistivity along the planes is quite low, and, in good pure single crystals and films of most materials, measured carefully, extrapolates nearly to zero. Qualitatively we may say

$$\rho_{res}^{ab} \ll \rho(T > T_c)$$

and

$$\rho_N^{ab}(T) \simeq BT$$

where the conductivity per layer per degree,  $B$ , is remarkably constant for good high  $T_c$  material. The numerical value is about  $\rho = (10 \pm 5)T$  ohms per layer. We discuss this intralayer conductivity first, going on later to the much smaller, if equally anomalous, interlayer conductivity.

It is a remarkably high conductivity. It is important to express it in terms of the Mott-Thouless-Yoffe "localization" parameter  $k_F l$ . The expression for conductivity in two dimensions may be written

$$\sigma = \frac{ne^2\tau}{m} = \frac{e^2}{2\pi\hbar} k_F l$$

when the universal "Mott" constant (sometimes called the von Klitzing conductance) is

$$\frac{e^2}{2\pi\hbar} = \frac{1}{25,000} \Omega^{-1}.$$

With  $\rho^{-1} = 10^{-3}$  at 100°K, we find  $k_F l \sim 25$ , very far on the metallic side of the Mott-Yoffe-Regel limit  $k_F l \sim 1$ . These materials are "good metals" in the *ab* plane, with *l*'s comparable to pure copper.

It is not widely enough recognized that numerical values of conductivity are very independent of complicated interaction effects. The Mott conductivity at the metal-insulator transition, as Mott himself emphasized, does not renormalize, and is quite universal experimentally and theoretically. It represents simply a coherence criterion as to how well and whether electrons can propagate by wave motion.

Its universality can be understood most easily in the context of quantum tunneling, and the relevant theorem was proved (I believe by Schrieffer) in the early years of quantum tunneling theory. It is easily shown using the tunneling Hamiltonian technique that the tunneling conductance into or out of a metal is proportional to the one-particle density of states per channel times the velocity of approach to the junction, summed over all the possible channels (for tunneling, read channel=momentum transverse to the junction)

$$\begin{aligned} \frac{dI}{dV} \propto \sum_{k_{\perp}^a} \sum_{k_{\perp}^b} \left( T_{k_{\perp}^a k_{\perp}^b} \right)^2 \times \\ \int_0^V dV' \left( \int dk_{\parallel} v_{\parallel} \text{Im} G_1(k, \omega = V) \right)_a \times \\ \left( \int dk'_{\parallel} v_{\parallel} \text{Im} G_1(k', \omega = V - V') \right)_b \end{aligned}$$

The integral is known as the tunneling density of states.

The essence of Fermi liquid theory is the idea that quasiparticle occupancies are in one-to-one correspondence with Hilbert space, and that low-energy properties may be calculated in terms of quasiparticles. Within this assumption, it is easily seen that the tunneling DOS is unrenormalized by dynamical effects. We insert  $v_{\parallel} = \frac{\hbar k_{\parallel}}{m}$  and obtain for the tunneling DOS at  $\omega$

$$\text{Im} \int_{-\infty}^{\infty} \frac{d\epsilon_k}{\omega - \epsilon_k - \Sigma(k, \omega)}$$



If  $\Sigma$  vanishes for large  $\omega - \epsilon_k$ , as it does (or at least goes to a constant), the contour may be closed and we simply pick up  $2\pi i$  at the quasiparticle pole (the singularities of  $\Sigma$  are on the other side of the real axis) if  $\omega$  is on the correct side of the Fermi surface, zero otherwise. Thus as far as transport is concerned, a quasiparticle acts as though it carries exactly  $e$  of charge and the density of states and velocity corrections exactly cancel out, even in many-body corrections.

The theory of quantum conductivity may be recast as a "tunneling theory" by generalizing the Landauer theory to many channels<sup>(1)</sup>: conductivity is a sum of transmission through channels, with the  $|T_{nn'}|^2$  factors representing the essentially geometrical scattering of the electrons by impurities or phonons which determines " $k_F l$ ". In this theory, it has always been noteworthy that the conductivity never participates in many-body renormalizations, due to the above theorem. The essential content of the theorem can be seen to be unitarity: the fact that particles cannot get lost, and if they enter the system in some channel they may be scattered but not lost.

One important exercise is to estimate the residual resistivity that a normal Fermi liquid would experience in the cuprate layers. In most of these materials, with  $YBa_2Cu_3O_7$  the only exception, there is very large non-stoichiometry. Let us take 20% doping as a typical, if low, value, realizing that in  $(La-Sr)_2CuO_4$  and in the *Bi* and some of the *Tl* compounds doping has been shown to be a consequence of nonstoichiometry.

The doping ions in general reside in the neighboring perovskite planes about 4 Å from the  $CuO_2$  layers. It is an overestimate to put the screening dielectric constant at 4 for the essentially insulating oxide material in the intervening non-conducting layer. The screening length in the layers due to the carriers themselves cannot be shorter than the distance to the nearest dopant, since the number of carriers = the number of dopants. We overestimate their screening by assuming that a dopant gives no potential if it is more than one lattice constant away from a given site and otherwise gives the full potential. A good estimate of the mean potential fluctuation from site to site is then

$$\overline{V^2} = .2 \times \left(\frac{e^2}{\epsilon_0}\right)^2 \times (1\text{\AA})^{-2} \times (4^{-2} + \frac{4.4^{-2}}{2} + 0)$$

$$\sqrt{V^2} \simeq .69 \text{ev}$$

This is the same order or somewhat larger than  $t$  as estimated by Schluter et al. Using, nonetheless, Born approximation, we would get

$$k_F l = k_F V_F \tau = 2t (k_F a)^2 \tau$$

$$\tau^{-1} = \frac{\langle V^2 \rangle}{4t}$$

so that  $k_F l \simeq 8(k_F a)^2 \frac{t^2}{\langle V^2 \rangle}$ .  $t^2$  is somewhat less than  $\langle V^2 \rangle$  so a good estimate is 5-10: about equal to the mean free path deduced from the resistivity as measured at room temperature.

A second guess just comes from  $l \sim \frac{1}{n\sigma}$  and assuming that  $\sigma = a^2$  as it must be since the potential fluctuation on the nearest site is as big as the bandwidth  $2t$ . This gives an  $l \sim 5a$ , in good agreement with the weak coupling estimate.

It hardly needs to be emphasized that in good, pure single crystals the residual resistance is at most  $10^{-1}$ , and often  $10^{-2}$ , of this estimated value. We approach values very like this only when doping with *Ni*, *Zn*, or other ions which substitute directly into the  $\text{CuO}_2$  plane positions. The argument is only strengthened when we look at estimates of possible resistivity due to phonons and to the rather large static positional disorder often encountered in these materials. These, too, are at least as large as the observed linear  $T$  resistivity and in spite of the considerable effort which has gone into fudging them to make them much more linear in  $T$  than is remotely plausible, one still is unable to reproduce such resistivity curves as those of Martin on *Bi* one-layer materials showing linearity from 10 – 700°K. The  $\text{Cu} - \text{O}$  layers have strong optical phonons which are known, from other data, to interact reasonably strongly with electrons (specifically, they shift when the energy gap opens up). There is no hint of irregularity in the  $T$ -dependence of resistivity near the appropriate temperatures. We show, in the Figures, some of the best data on *ab* plane resistivity, especially that of Ong, et al on YBCO, Martin on *Bi* materials, and Ginsberg et al on YBCO, to illustrate these points. A preliminary curve on  $(\text{La} - \text{Sr})_2\text{CuO}_4$  is also shown.

We will discuss infrared results more thoroughly in the spectroscopy chapter, but it is worth saying here that the best evidence is, in the first place, that  $\sigma(\omega \rightarrow 0)$  extrapolates well to the observed d.c. conductivity values, and second, that consistent with  $\sigma \propto \frac{1}{T}$ , for  $\omega > T$  we find, out to  $\sim 1000 \text{ cm}^{-1}$ , that  $\sigma \propto \frac{1}{\omega}$ , quite accurately, with no observable features in this region.

A recent measurement shown in Fig. ( ) by Ginsberg et al has given us the *ab* anisotropy in YBCO. It appears that there is an appreciable conductivity due to the chains in the normal state but that both chains and planes satisfy

$$\rho_{||} \propto T$$

with residual resistivity  $< .05 \times \rho(T = 100^\circ)$ .

The planar metallic conductivity in all its strange aspects leaves us convinced that in some sense the normal state of the planar layers has superconducting aspects. The conductivity is not affected by impurity or phonon scattering, empirically seeming to be immune to  $T$ -invariant scattering as is BCS superconductivity. It would be a valid description of the empirical facts to say that the conductivity behaves like fluctuation conductivity from superconducting fluctuations above a superconducting state at  $T_c = 0$ , with an exponent -1 as in conventional 2d fluctuation conductivity,  $\sigma \propto \frac{1}{T - T_c}$ . The coefficient, however, bears no resemblance to that in conventional fluctuation theory. Nonetheless these facts are powerful arguments that whatever the nature of the superconductivity, it leads to  $T$ -reversal invariant—i.e., BCS—pairs.

A second set of data are equally persuasive on this point. Many measurements, for example those of Ong shown in Fig. ( ), demonstrate that certain impurities do succeed in strongly modifying the planar conductivity in such a way as to produce a residual resistivity, while, as we know, many other dopants have an essentially negligible effect. Such dopants are *Ni*, *Zn*, *Co* and *Ga* in YBCO, *Pr* in BISCO, among other cases. These are also the impurities which strongly reduce  $T_c$ , in much the same proportion. There seems to be a monotonic relationship between  $\rho_{res}$  and  $T_c$  in all these cases.  $\rho_{res}$  also appears in the oxygen deficient YBCO with reduced  $T_c$ .

Recently several measurements have shown that the dopants which reduce  $T_c$  and increase  $\rho_{res}$  have the following two properties:

- (a) they are substitutional for Cu in the planar sites.
- (b) they induce a free spin moment in the planes, as measured by magnetic susceptibility and, recently, by electron paramagnetic resonance (Ong, Hong, Cieplak and Finkelstein, et al). The susceptibility and resistivity measurements are quite quantitative.

There are two points to these measurements, one straightforward and the other not quite so. Straightforwardly, it is clear that spin scattering is in some sense "pair-breaking": it does cause resistivity, as well as lowering  $T_c$ . We may describe the effect straightforwardly in our " $T_c = 0$ " superconductor language by saying that " $T_c$ "  $\propto -n$ , i.e.  $T_c$  is reduced by the number of pair breaking scatterers, and then  $\sigma$  is still  $\propto \frac{1}{T-T_c}$ . Again, this is precisely the conventional behavior of BCS superconductivity, except for the large magnitude of the effects. This effect, almost more than anything else, proves that the conductivity is not conventional, in that there is a complete contrast between  $T$ -invariant and  $T$  non-invariant scattering, which is not compatible in any reasonable way with the hypothesis of a conventional Fermi liquid. A somewhat more subtle question is why Zn, a "non-magnetic" impurity, gives the same spin that Ni, a nominally magnetic impurity, does. Here is a straightforward experimental case of "hole-particle symmetry": adding one relatively + or - charged ion in the  $Cu^{++}$  lattice has the symmetric effect of adding a localized spin. The real meaning of this observation will become clear later when we come to discuss "confinement". But we can see intuitively that the spin manifold, which we can think of as spinons moving about on the manifold of singly-occupied ( $Cu^{++}$ ) ions, is behaving to a great extent like a Fermi gas, and that a local potential creates a singly-occupied bound state for a spinon and its accompanying holon or antiholon, the two forming effectively a localized electron or hole, carrying both spin and charge.

The  $ab$  plane conductivity is, as described above, bizarre; but it is nowhere near so puzzling as the  $c$ -axis and intergranular contact resistivity. As I remarked, especially the high resistance of intergranular contacts in the normal state was noticed very early, but

with the advent of single crystals it became clear that even the bulk conductivity has a truly gigantic anisotropy. The conductivities along the *c*-axis of respectable single crystals of  $(La - Sr)_2CuO_4$ ,  $YBa_2Cu_3O_{7-\delta}$ ,  $Bi\ 2212$ , and some newer materials all are 100 to  $10^4$  times as low as in the *ab* plane. Since, as we showed, the *ab* plane resistivity is only a factor 10–20 below the Mott limit even at 100°K, this means that the *c*-axis resistivity is nonmetallic in all cases.

More striking still is its *T*-dependence, especially in the case of the best single crystals measured carefully, emphasizing this parameter. In general, a good fit to most data is

$$\rho_c = \frac{A}{T} + BT$$

with the lowest temperatures dominated by the  $A/T$  term. Fig. ( ) is a sequence of samples of YBCO measured by Ong showing the the  $BT$  term is sample-dependent, while the  $A/T$  term is reasonably constant. One possible explanation for such an expression is that actually the interlayer resistivity is even higher than that which is measured but much of it is bypassed by devious transverse paths. A characteristic picture, which I do not actually think is unrealistic, is of a screw dislocation or growth spiral or a sequence of interleaving antiphase boundaries which allow parallel conductivity to propagate in the *c* direction in series with a genuine *c*-axis region. Intergranular contacts have the same  $1/T$  resistivity characteristic, as is seen from a  $\rho T$  vs  $T^2$  plot (Fig. ( )) for the original Müller-Bednorz data on poorly compacted ceramics.

These data are often rationalized away on the basis of a number of suggestions which are unrealistic. Let us discuss each possible alternative in turn.

- (1) The electrons are "localized" in the *c* direction, extended in the *ab* plane. This represents a misunderstanding of the nature of localization. If a wave-function is extended in any direction it has matrix elements to a continuous spectrum of final states in all other directions and is hence extended in all dimensions. "Extended" and "localized" states cannot coexist.
- (2) The relevant one-electron matrix elements are just too small to give coherent motion in the *c*-direction, and all motion is phonon-assisted. Entirely aside from the fact that

the resistivity often fits a smooth  $1/T$  plot indicating no Debye or Einstein phonon bumps (which often appear in phonon-assisted tunneling), the interplanar hopping can be quantified in several ways.

- (a) Band theory gives us values for the effective masses in the  $c$ -direction which are not negligible either in  $La_2CuO_4$  or in  $YBCO$ . In the former the hopping matrix element is predicted to be about .1 ev, in the latter, that between the close planes is  $> .1$  ev, between chains and planes about .02 – .05 ev. Only in BISCO is the hopping via the Bi layers truly small.
- (b) In  $La_2CuO_4$  and  $YBCO$  the values given by band theory are confirmed experimentally to be correct in two ways: values of superexchange integrals  $t^2/U$  which using reasonable  $U$ 's give the above values for  $t$ ; and superconducting penetration depths for  $c$ -oriented currents. Malozemoff's data, for instance, give a mass anisotropy of at most 10 in  $YBCO$ . This is perhaps the strongest and most significant single experimental anomaly: the  $c$ -axis one-electron matrix elements are clearly present below  $T_c$ , but fail to cause coherent transport in the normal state. This failure of the conventional sum rule is the key clue to the mechanism of superconductivity.
- (3) A third suggestion, which we have already, in effect, dismissed, is that there would be large many-body (" $Z$ ") corrections to the coherent transport in the  $c$ -direction. This is one of the reasons we emphasized the generality of the proof of non-renormalization of transport processes in Fermi liquid theory.

In general mass and density of states corrections cancel exactly, so that conductivity is usually much more isotropic than effective masses. What then happens in a true tunnel junction? In fact, the relevant scale of energy is the inelastic scattering time: if  $t_{\perp} < \hbar/\tau$ , we get tunnel junction behavior, which is still temperature-independent but can lead to a resistivity greater than the Mott limit. In several of these materials we know that  $t_{\perp} > \hbar/\tau$ , and in none of them is the behavior like that of a stack of tunnel junctions.

This kind of highly anisotropic conductivity is not unknown, but in most cases the

cause is essentially the same as here: there is, in the low-dimensional material, a highly correlated state, SDW or CDW, and the charge carriers are not the tunneling entities—electrons—but some form of soliton. As we shall see, it is the exceptional 2 or 1-dimensional material which is a true Fermi liquid, and this case is not that exception.

The only reasonable explanation for the peculiar *c*-axis transport is to abandon the hypothesis of the Fermi liquid. We are already happy to do that to understand  $\rho_{ab}$ , we must do it to understand  $\rho_c$ , because we must abandon the structure in which the quasiparticle states arise by adiabatic continuation from real particle states; i.e., we must abandon the definition of the incoming particle channels. This is what happens in the BCS theory of superconductivity: the quasiparticles become mixtures of hole and particle states, and an incoming electron at some point must encounter a reflection, clearly demonstrated by Andreev scattering.

It is perhaps a somewhat high-flown way of describing what happens but it is a valid analogy to call the process “confinement”. In particle physics quarks can only live in the “real”, bare vacuum which is not the “physical” vacuum we have all around us; this latter is a complex many-body state containing large amplitudes of gluon fields, etc., and in it only conventional hadrons can move freely. Quarks build themselves a “bag” of real vacuum inside the nucleon. Yet there is no true symmetry breaking or order parameter in the physical vacuum. In our case, it appears that electrons can only live in the bare vacuum which we have all around us, and which also inhibits ordinary metals and the intervening layers of insulator. They are the fundamental particles, the “quarks” of the theory. Inside the  $\text{CuO}_2$  planes is a “physical vacuum”, a correlated state in which electrons are not elementary excitations, but completely unstable: they break up into something else. This is not unfamiliar to us—it is exactly what happens in BCS superconductors, and in charge-density wave systems as well—but it is unfamiliar that it happens in the absence of a symmetry-breaking order parameter such as the BCS gap. Fortunately we have one clear example of this process to provide a model—and very possibly the model—for what happens, namely the exactly soluble 1-dimensional Hubbard model and its near relatives

such as the " $t - J$ " model and some of the one-dimensional "g-ology" models solved in the '70's by renormalization techniques.

In these models a process called "spin-charge separation" takes place, a process in which the elementary excitations become collective, soliton-like motions of the entire electron fluid, and in which the motions of the charge and spin degrees of freedom become decoupled, in that they have different velocities.

It is not valid to say, as is often implied, that they are "completely" decoupled: clearly the two fluids represent the same set of electrons, and they must, on the average, flow together. This appears in the dynamics as a "backflow" coupling of the two sets of excitations, a very long-range but strong condition on the overall flow. This can often be well described in terms of a gauge field.

Thus we have two separate sets of soliton-like excitations, which we may call "spinons" and "holons", and which are independent degree of freedom in the layers. These must coalesce into an electron in order to tunnel through into the adjacent layer, and will then break up again, and we have no reason to expect the conventional transport theory to hold for such a process.

Until recently this picture was our only guide. Now that we have a reasonably clear picture of the one-dimensional model, one may say a bit more. In the first place, it is important to take the correct limit, since the wrong conclusion is ensconced in the literature because of calculations naively using invalid perturbation theory. In the usual, incorrect calculations, it has been customary to turn on the interchain or interlayer hopping

$$t_{\perp} \sum_i (c_i^+)_{\alpha} \cdot (c_{i\sigma})_{\beta} + hc$$

first, and then to do perturbation theory in  $U$ , which of course must then be resumed by renormalization group theory or otherwise, a process which cannot be controlled formally and does not necessarily lead to a valid large  $U$  limit. Naturally, we then are doing perturbation theory on a state in which the bands have been split by  $t_{\perp}$  and the Fermi surface modified. For instance, the two-chain problem will have been split into two one-



chain problems with different  $k_F$ 's which do not interact at the low frequency end. Thus in this order turning on  $U$  has no effect.

The correct order is to turn on  $U$  and only then to introduce  $t_{\perp}$ . Only in this order is it possible to test in a rigorous fashion whether a small  $t_{\perp}$  can modify the band structure or not. In fact, in 1d it is easy to demonstrate the contrast. The relevant ground-state diagram expressing the energy response to  $t_{\perp}$  is a loop consisting of a tunneling process at  $x = 0$ ,  $t = 0$ , an electron and hole propagating in chain  $a$  and chain  $b$  respectively to  $x, t$ , and a particle tunneling back at that point:

$$D = \int dx \int dt |t_{\perp}|^2 G_1^a(x, t) G_1^{*b}(x, t) .$$

In the case of non-interacting electrons, this is

$$\begin{aligned} & |t_{\perp}|^2 \int dx dt \frac{1}{x - vt + i\eta} \frac{1}{x - vt - i\eta} \\ &= \frac{|t_{\perp}|^2}{\eta} \int dx dt \delta(x - vt) \end{aligned}$$

which diverges as  $\frac{1}{\eta}$ , signalling the fact that there is a finite tunnel conductance or, equally, the fact that there is an energy shift at the Fermi surface of  $\pm t_{\perp}$ , linear, not quadratic in  $t_{\perp}$ . The motion between the two chains is, in this case, coherent.

In the case even of weakly-interacting electrons, the Green's function changes qualitatively. When spin and charge separate, they move at different velocities, and also a collective charge wave moves in both directions from the point where a particle is inserted.

$$G = \text{const} \times (x - v_s t + i\eta)^{-1/2} (x - v_c t + i\eta)^{-1/2} (x^2 - v_c^2 t^2)^{-\epsilon/2}$$

In this case,

$$GG^* \propto \delta(x - v_s t) f(x - v_c t)$$

where  $f$  does not even have any amplitude at  $x = v_s t$ . Clearly the divergence which appears in the non-interacting case has been replaced by, effectively, zero. A small  $t_{\perp}$  leaves the two chains completely isolated from each other, only able to communicate when thermal

excitations are present, and certainly not capable of coherent exchange of single particles. This is the process we describe as "confinement" and it is implicit in any treatment in which charge and spin separate and, in fact, takes its origin in the gauge couplings of charge and spin solitons. But it is, in effect, very simple; for infinite-time, coherent processes, the electron—which is the only particle which can tunnel through the vacuum—separates into its constituent particles and these eventually get infinitely far apart and cannot recombine to tunnel back.

We must assume that spin-charge separation occurs also in the 2d case and is responsible for the insulating c-axis behavior and for the poor intergranular contacts in the normal state. In a later chapter we will give considerable theoretical justification for this. This argument is also crucial, as we shall see, to the mechanism for superconductivity, which involves coherent pair ("Josephson") tunneling, which is possible at zero frequency, since it can take place in the singlet channel without generation of any real spinons.

A third puzzling experimental fact is the temperature-dependent Hall effect. Fig. ( ) shows measurements on a series of ceramic Ni-doped samples by Clayhold et al, which are fairly typical. Relatively few good single-crystal data exist.  $YBCO_7$  and  $Bi\ 2212$  both show remarkably steep temperature dependence,  $R \propto \frac{1}{T}$ , which is reflected in the ceramic measurements. It is supposed on the basis of ceramic data that this is exceptional, but we are not at all sure whether or not this is the case.  $(La - Sr)_2CuO_4$  shows a more normal Hall effect but it still has a strong  $T$ -dependence, and in fact because the magnitude of  $R_H$  (which in principle is  $\propto \frac{1}{N}$ ) is bigger in this material, the  $1/T$  term is roughly equally big. The electron-doped material  $Ce$  --- shows a large Hall effect with rather spectacular  $T$  and sample dependence, which to us suggests either questionable samples or a composite band. We do not attempt an interpretation of this data.

In the more usual case, it seems to us that a rather simple interpretation is available. This depends on the fact that the resistivity mechanism is indirect: resistivity is not caused by scattering of the quasiparticles carrying the charge, however else one wishes calculate it—either from fluctuations of the gauge field, decay of accelerated electrons, or

otherwise. The actual Fermion "quasiparticles" are the spinons, which are in general much less scattered than the observed resistivity would indicate, because they are independent of the charged, singlet channel; but it is these Fermions which determine the widths of states near the Fermi surface, and thus determine the  $\tau$  which should be used in the Hall angle  $\omega_c \tau$ . That is, the momentum states from which the collective state is composed are rotated under the action of the magnetic field by the equation of motion

$$\hbar \dot{k} = \frac{v}{c} \times B$$

and the dynamic memory of the last acceleration lasts a time  $\tau$  given by the energy width of eigenstates near the Fermi surface, not the energy width of the electron states which determines  $\rho_{ab}$ .  $1/\tau$  for the spinons contains two types of scattering; both involving only spin scattering since  $T$ -invariant scattering is ineffective. One type is spinon-spinon scattering which is identical to conventional electron-electron scattering and has  $T^2$  temperature dependence; the second is whatever magnetic scattering is responsible for the residual resistance if any. Thus we have

$$\frac{1}{\tau} = \frac{1}{\tau_{res}} + AT^2$$

and

$$\theta_H = \omega_c \tau = \frac{\omega_c \tau_{res}}{1 + AT^2 \tau_{res}}$$

This gives a Hall effect of the form

$$\begin{aligned} R_H &= \frac{\rho_{ab} \theta_H}{H} \\ &= \frac{\omega_c \tau_{res}}{H} \cdot \frac{\rho_{ab}}{1 + \tau_{res} AT^2} \end{aligned}$$

Now

$$\rho_{ab} = \frac{m}{ne^2} \left( \frac{a}{\tau_{res}} + BT \right)$$

so that

$$R_H = \frac{1}{nec} \times \frac{1 + BT \tau_{res}}{1 + AT^2 \tau_{res}}$$

Clearly, if

$$\frac{1}{\tau_{res}} \rightarrow 0 \quad \text{this gives} \quad R_H \propto \frac{1}{T}$$

If  $\frac{1}{r_{xx}}$  is finite, we find a characteristic temperature dependence which had previously seemed quite puzzling. Fig. ( ) shows a fit of this temperature dependence to a sequence of Ni-doped YBCO samples measured by Clayhold et al. It is very interesting that the two cases in which we have good measurements on single crystals of very high  $T_c$  superconductors both show a pure  $1/T$  Hall effect (see fig. ( ), where  $n_H = \frac{1}{R_H} \propto T$  is plotted). Our prejudice is that in fact the linear  $T$  behavior of  $n_H$  is in some sense the characteristic behavior, and that the apparently more normal behavior of many samples is a consequence of spin-scattering impurities. This prejudice needs to be tested against many more samples, especially of good single crystals.

So far, the measurements of other transport effects are too fragmentary to be very helpful. Heat conductivity and thermopower are so anisotropic and so difficult to interpret for ceramics that we choose to ignore all ceramic measurements. Heat conductivity seems to obey a Wiedemann-Franz law in the  $ab$  plane, with some hint that the coefficient is a bit bigger than 1. There is a puzzling jump in heat conductivity below  $T_c$ , suggesting that  $ab$  plane phonons are strongly scattered in the normal state, and this is consistent with the observation that there is no  $T$ -dependent heat conductivity in the normal state which behaves like a phonon conductivity. This is not incompatible in principle with the observation that electrical current flow is not affected by phonons: the scattering can involve fluctuations in which the charge and spin excitations are counterflowing, i.e., a "normal fluid" component—essentially, spinons counterflowing against holons. But we must not pretend we understand these observations. In the  $c$ -direction, the heat conductivity is  $T$ -dependent and of a magnitude quite compatible with phonon conduction, and no anomaly occurs at  $T_c$ . The observations on YBCO are shown in Fig. ( ).

Thermopower measurements on single crystals are even more fragmentary. In the early days of high  $T_c$  one was reassured that the carriers were holes by the thermopower sign, which is quite immune to the kind of band structure problems that the Hall effect has. Otherwise we have seen no clear, reproducible trends, partly because of the absence of generally agreed data. This is a pity because in many cases thermopower is the easiest

measurement to interpret. The present picture can make fairly unequivocal predictions as to both  $c$  and  $a$ -axis behavior, since we have a clear picture of both transport processes. As far as we can see,  $ab$ -axis should, like the thermal conductivity, behave normally, because of the backflow condition: a spinon must flow for every holon or vice versa.  $c$ -axis should be more interesting: because high-energy particles hop more easily, proportionally to  $\omega$ ; we expect a  $T^2$  behavior. Both predictions are not as yet firm, and both seem to be borne out more or less by what experiments there are. Both of these effects can be studied on the one-dimensional model but have not been; this is a good area for study.

Tunneling is a transport process but we shall rather arbitrarily assign it to the spectroscopy chapter. It was, of course, the first hint that non-Fermi liquid behavior was present, but to this day tunneling measurements are ambiguous and controversial. There is one remark we should make: many of the great difficulties with tunneling measurements could well fit under the general rubric of "confinement" effects. We still have no clear evidence that even normal metals can tunnel into a high  $T_c$  materials in a normal, coherent way along the  $c$ -axis; there is a mysterious "dead layer" on the surface of every sample which could perfectly well be the confinement effect. Certainly intergranular contacts are sources of resistivity, especially in the normal state.